

beyond dispute is that our teachers have wretched text-books in geography, and Germany has been held up to us as the model to follow in this as in many other respects in regard to geographical teaching. The Germans (as Mr. Chisholm points out in his interesting preface) have had long experience in working out an advanced system of education; they know that a limited period must be turned to account for the thorough teaching of a great variety of subjects, and accordingly they have learned to distinguish between what is indispensable as a groundwork and what must be omitted. In this country the study of geography is mainly a work of memory—the names of towns, rivers, mountains, with their populations, lengths, and heights. This and similar details are precisely those on which the Germans lay least stress, and as Mr. Chisholm has “earnestly endeavoured to guide himself by German examples,” he anticipates that his book will appear more remarkable for what it omits than what it contains. Stated in his own words his object has been, in the first place, to draw a mental picture of the different countries and regions of the world, giving due relief to what is most distinctive in each region, and, secondly, to give special prominence to the relation of cause and effect, so as to enable pupils to realise that in geography there is something to understand as well as to commit to memory, in other words, to make geography a mental discipline as well as a body of instruction. Of course there is important work for the memory in geography as in every other branch of education, and this the author recognises, and provides for in his tables and printing. He insists, too, on the vital necessity of maps, without which there can be no adequate knowledge of geography. A text-book is supplementary to an atlas, and does not supersede it. These are high ideals which Mr. Chisholm sets before him; let us see how he fulfils them.

The whole volume contains 320 pages. The first 60 are devoted to an introduction dealing with mathematical and physical geography, which, as explained in the preface, is designed primarily for teachers, and is not intended to form part of the course for the pupils until they have gone through the whole body of the book.

The introduction is followed by a description of continents and countries. Of the 260 pages which remain for this purpose, Europe fills 150 pages, Asia 45, Africa 16, and America 32. The proportions are based on the degree of knowledge which an educated English boy or man should have of the respective countries and continents. Some of the divisions are original. Thus English counties are divided into corn and grazing, the countries of Asia into monsoon and non-monsoon countries. We have specially examined the sections devoted to the countries of Eastern Asia, for the sins of ordinary British school geographies are more apparent here than elsewhere—the sins, namely, of stereotyped inaccuracy, and of strings of names and numbers. Mr. Chisholm has not a superfluous line in any of these sections, the information is of the latest kind, and all the knowledge that the average boy requires of the countries is put in a short space.

As an instance of the care with which the work is done it may be mentioned that the puzzling variations of some Japanese names (*e.g.* Fujinoyama, Fujisan) are given and explained. On the whole, we are convinced that there is at present no school geography in the English language more calculated to give adequate and intelligent instruction in that subject than this, and can therefore strongly recommend it to those teachers who have lamented the absence of a sound text-book. It is to be hoped that Mr. Chisholm may see his way to producing a smaller work about half the size and price of this book for lower classes.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.]

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.]

Physiological Selection and the Origin of Species

AS Mr. Romanes has referred to my article in the current number of the *Fortnightly Review*, and stated that he is prepared to answer what he terms “the very obvious exceptions” which I have taken to his theory, I shall be glad to be allowed to state, very briefly, what those exceptions are, and to give an illustration of one of the more important of them.

(1) Mr. Romanes makes a great deal of the alleged “inutility of specific characters,” and founds upon it his extraordinary statement that, during his whole life, Darwin was mistaken in supposing his theory to be “a theory of the origin of species,” and that all Darwinians who have believed it to be so have blindly fallen into the same error. I allege, on the contrary, that there is no proof worthy of the name that specific characters are frequently useless, and I adduce a considerable series of facts tending to prove their general utility.

(2) In support of his view as to the swamping effects of intercrossing, Mr. Romanes objects to the assumption of Darwin, “that the same variation occurs simultaneously in a number of individuals,” adding: “Of course, if this assumption were granted, there would be an end of the pre-ent difficulty”; and his whole argument on this branch of the question rests on the assumption being false. I adduce evidence—copious evidence—that the supposed assumption represents a fact, which is now one of the best-established facts of natural history.

(3) Mr. Romanes states, as the special feature of his physiological varieties, that “they cannot escape the preserving agency of physiological selection.” He gives no particle of proof of this, while I show that, on the contrary, it is hardly possible for them to survive to a second or third generation. It is on this point that I wish to give an illustration. Mr. Romanes speaks of his supposed variations as “showing some degree of sterility with the parent form,” while continuing to be fertile “within the limits of the varietal form”; but I hold that any such variety (beyond single individuals) can hardly exist, while he has adduced no proof whatever of their existence. To show the improbability of their existence, let us suppose a definite case.

In a given species there is born an individual, A, which is infertile with the bulk of the species, but fertile with some few individuals of the opposite sex, *a, b, c*. Let there be a second individual, E, born from other parents in another part of the area occupied by the species, and fertile only with *e, f, g*. Other individuals, K, P, R, &c., may have similar relations, each infertile with the bulk of the species, fertile only with a few individuals which may be termed their physiological complements. Now each of these, separately, is a physiological variety, but the whole set, A, E, K, P, R, do not form one, but five distinct varieties. To form one variety all of them must be fertile with the same identical set of individuals of the opposite sex, and this seems to me to be so highly improbable that it must not be assumed till rigidly proved. Yet there is not one passage in Mr. Romanes' paper to show that he recognised this difficulty; on the contrary, he always speaks as if any number of separate physiological variations within one species must necessarily form one variety. It will easily be seen that the chances against any single variety of this nature being preserved are overwhelmingly great. For, first, at least two of the complementary individuals must survive to the breeding-season, and the chances against this are measured by the fertility of the species. If it produces ten young each year, the chances are between nine and ten to one against any one of them surviving. The chances against the two complements surviving will be about ninety to one; and then there remains the chances against the two meeting at the breeding-season, for, by the assumption, there is nothing whatever to bring them together but chance, and this may be any number of thousands to one.

There are, no doubt, other possible cases in which the physiological variety might be continued, but, as I have shown in my

paper, the chances against it are always very great. Here, then, are three objections to Mr. Romanes' theory which seem to me to be weighty and fundamental; yet he says, in effect, that he anticipated, and is prepared to answer, them. This, I must say, puzzles me; because in the whole of his lengthy paper, occupying seventy-five pages, I cannot find any adequate recognition of their existence, or any attempt whatever to answer them.

My apology for writing this is that I am shortly leaving England, and wish the readers of NATURE, who may not have seen the *Fortnightly*, to be aware of the character of the objections which Mr. Romanes declares that he anticipated, but apparently thought of too little importance to require any discussion in his paper.

ALFRED R. WALLACE

I AM sorry that I have not succeeded in making my meaning clear to Mr. Romanes. I had hoped that my former letter (NATURE, September 2, p. 407) would have given some indication as to my father's views. With regard to the sentence quoted from the "Origin of Species," our views seem to differ so much that it seems useless to prolong the discussion.

FRANCIS DARWIN

Golf Club, Felixstowe, September 13

I HAVE read the numerous notes and letters in recent numbers of NATURE upon the origin of species and varieties with great interest. It seems to me that all your correspondents are raising an imaginary difficulty.

"If it is to the advantage of some particular variety not to resemble the parent form," then that variation must have been produced by some efficient cause acting upon the parent form alone. Is it not obvious that that cause still acting will be still more potent in producing that particular variation when the parent form intercrosses with the variety? This is, of course, supposing that the new variety is suitable to its environment; if it is not so, no amount of "propping up," whether by "amixia" or otherwise, would perpetuate it.

If, as is probably the fact, varieties or incipient species have arisen from individual divergences, amixia would tend to immediately suppress them in the case of animals and dioecious plants, as a new generation could not possibly arise without intercrossing with the parent stock.

J. H. A. JENNER

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I SHOULD be glad to call Mr. Romanes' attention to a letter by Mr. Edmund Catchpool, published in NATURE, November 6, 1884 (vol. xxi. p. 4), where he will find his theory of physiological selection very clearly put forward.

FRANK EVERSHERD

113, Darenth Road, Stamford Hill, N.

Solution Discussion at the British Association

It was a pity there was no discussion on solution in British Association, Section B, on Thursday last. More than the whole day was taken up with reading a great many papers, some of them having very little to do with the subject, so no time was left for discussion. I was indeed, by the courtesy of the Vice-President and the patience of the few remaining listeners, allowed to make a few remarks, but of course it was only possible for me to indicate that I had something to say.

In the papers referred to a good deal was said of solution being due to purely physical causes. Now this is either a truism or a veil to hide ignorance, and I am sure no one was a bit the wiser. What we want to get at is THE physical cause of solution. Again, a great deal was made of the part of the heat of solution that might be accounted for by the contraction in volume of the solution. This looks very learned and scientific, and no doubt is interesting from some points of view, but even if all the heat could thus be accounted for, it would not advance our knowledge of the cause of solution; it is merely surrounding the subject with cobwebs. The question would still remain, What is THE physical cause of this contraction?, and I maintain it is due to the affinity of all the elements for one another acting as pointed out in my papers on chemical affinity and solution published in NATURE, April 29 and July 22 of this year. The truth is, chemists, for convenience of study, drew a circle and called all within this "chemical affinity," and then

forgot that the circle was their own making, and imagined it was Nature's work. This restriction has served its day, and must now be obliterated if we would understand the plainest teaching of the laboratory and make continued progress.

Portobello, September 9

WM. DURHAM

Actinotrocha of the British Coasts

IN NATURE of August 19 (p. 361), which I have only seen to-day, my friend, Mr. J. T. Cunningham, records as a novelty the finding in 1883 of *Actinotrocha* off Cronarty Firth. Without giving an exhaustive note of its occurrence off our shores since the discovery in 1856 of *Phoronis* by the late able and accomplished Dr. Strehill Wright, viz. one species from Ilfracombe, and another on an oyster-shell from the neighbourhood of Inchkeith in the Firth of Forth, the following remarks may be of interest. So long ago as 1858 the late Dr. Spencer Cobbold found *Actinotrocha* near Portobello, as was likely after Dr. Wright's discovery, and I have also since met with it in and off the Forth. Moreover, at the meeting of the Microscopical Society at which Dr. Cobbold read his paper, the lamented Dr. Carpenter mentioned that he had found *Actinotrocha* in abundance off the Island of Arran, probably when working at *Tomopteris* and other surface-forms with his friend, the enthusiastic E. Claparède, of Geneva. Besides these localities, Prof. Kölliker ("Kurzer Bericht an der westküste von Schottland," *Zeitsch. f. w. Zool.*, Bd. v. 1864) describes the occurrence of a *Phoronis* apparently identical with Dr. Wright's *P. hippocrepia* from Millport on the larger Cumbræ in the Clyde, a region in which the steam-yacht *Medusa* from Granton has lately been at work. It is probable, indeed, that *Phoronis* and its larval form (*Actinotrocha*) are more generally distributed round our shores than the scanty notices of them would lead one to suppose. Old shells in and off the mouth of the Forth, off the western shores, and these and other structures in the littoral region on the southern coast of England, as well as the shores of the Channel Islands, will probably produce many examples of *Phoronis*, while the careful scrutiny of the contents of the tow-net in similar localities will yield corresponding results as regards *Actinotrocha*.

W. C. MCINTOSH

St. Andrews Marine Laboratory, August 25

The Manatee

I NOTICE in the review of Dr. C. Hartlaub's work on the Manatees, which appears in your issue of July 8 (p. 214), that the geographical range ascribed to that animal on the West Coast of Africa has its southern limit at the Quanza. A reference to earlier writers would, I think, justify us in believing that the manatee was once to be found as far south as the Cape of Good Hope, or else that it has been confounded with the hippopotamus.

Dapper, in his description of the Cape Settlement, speaks both of sea-cows—"zee-koeien of zee duivels, zoo groot als koeien, die bij wijlen te lande gaen weiden"—and of sea-horses—"zee-paerden, een zeer groot en wonderigelyk zee-gedrocht" ("Naukenge Beschijonige der Afrikaensche gewesten," p. 266; Amsterdam, 1676).

Here the hippopotamus is evidently the *zee-koe* or sea-cow, which occasionally feeds on dry land. May not the *zee-gedrocht*, the sea-monster, have been the manatee?

For Valentyn, also writing of the Cape of Good Hope, refers very explicitly to the manatee:—

"Onder de zee dieren telt men de zee koeien, de hier zeer veel en ongemeen swaar vallen, alzo men er zommige van 4 of 5000 ponden gezien heeft. In West Indien noemt men dit dier *Manati* bij de Indianen, en anderen noemen het wel een Lamantine; hoewel er zijn die beide deze dieren nog eenigzins onderscheiden.

"Diergelijk zwaar zee paarden heeft men er ook, hoewel wat verder van de Kaap af, gezien. Zij vallen doorgans kastaniebruin" ("Beschrijving van Kaap de Goede Hoop," p. 115; Dordrecht and Amsterdam, 1726. Eighth volume of "Oud en Nieuw Oost Indien").

But here the manatee is called the sea-cow. What is the sea-horse (*zee-paarden*)? Can it be what Leguat saw at sea on his voyage from Amsterdam to the Cape—which he reached twelve days after the *rencontre*?

"Le premier jour de l'an 1691 nous eûmes le plaisir de voir assez distinctement une vache marine de couleur roussâtre (cf. the "kastaniebruin" of Valentyn) "qui faisoit voir la tête entière,